

3 (2)

A

DISSERTATION

ON

RESPIRATION.

TRANSLATED FROM THE LATIN OF

DR. MENZIES.

WITH NOTES,

BY CHARLES SUGRUE,

HONORARY MEMBER AND PRESIDENT OF THE ROYAL
PHYSICAL SOCIETY, AND FORMERLY PRESIDENT
OF THE AMERICAN PHYSICAL AND CHI-
RURGO-PHYSICAL SOCIETIES OF
EDINBURGH.

EDINBURGH:

PRINTED FOR G. MUDIE AND SON—AND FOR
J. JOHNSON, LONDON.

1796.



P R E F A C E

PHYSICIANS were, till very lately, totally ignorant of the effects of respiration on the system: Modern chemistry, by discovering the component principles of atmospheric air, and that portion of it which underwent a change in the lungs, induced philosophers to examine the subject with the degree of attention it merited. The names of Black, Crawford, Lavoisier, &c. will be transmitted to posterity, as the first who have elucidated this important point. The following Dissertation will be found to contain experiments made with the greatest degree of accuracy; and the conclusions are such as evidently flow from them. This, together with the importance of the subject, will, it is presumed, be a sufficient excuse for giving it to the public in an English dress.

If we consider that the better the functions of any organ are ascertained, the more effectually we shall be able to prevent or cure its diseases, and that no organ is subject to more dangerous or obstinate diseases than the lungs, it will be evident that whatever throws light on so interesting a topic is worthy of our attention.

Two other motives, which influenced the Translator, are, that the Latin edition is extremely rare; and that the experiments and conclusions of Dr. Menzies have met with the approbation of such of the Professors of this University, as treat on the subject in their lectures.

The notes added by the Translator, are marked with capitals; the others are chiefly references to authors.

TO
JOSEPH BLACK, M. D.

PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF
EDINBURGH.

&c. &c. &c.

SIR,

I COULD not long hesitate to whom I ought to dedicate this dissertation, when I considered the numerous discoveries with which you have enriched science, particularly that of animal heat, of which your ingenuity had, twenty years ago, discovered the source. This, Sir, has spread your reputation throughout the world, and makes you be considered as the Father of modern Chemistry.

I ought not, among other motives, to omit the inestimable advantages I have derived from your lectures; nor shall I ever forget the friendship and politeness you have shewn me.

I remain with all due respect,

S I R,

Yours, &c.

Sep. 6th, — 1790.

ROBERT MENZIES.

JOSEPH H. BLACK, M.D.

PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF

CHICAGO

I could not help feeling in some degree
embarrassed by this distinction, when I considered
the numerous diseases with which you have
endeavored to contend, particularly that of animal
poison, of which you have recently had twenty years
experience. This, however, I have not
your reputation in the world, and
make you be considered as the father of mo-

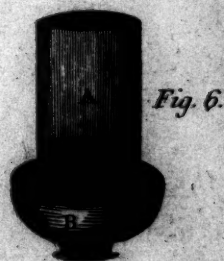
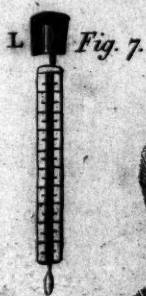
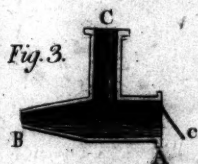
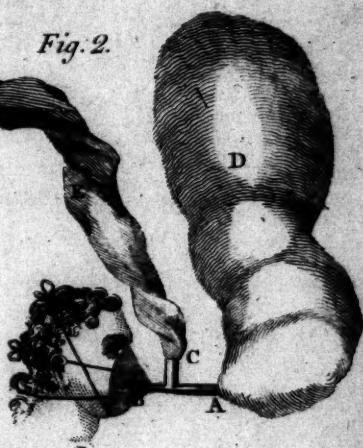
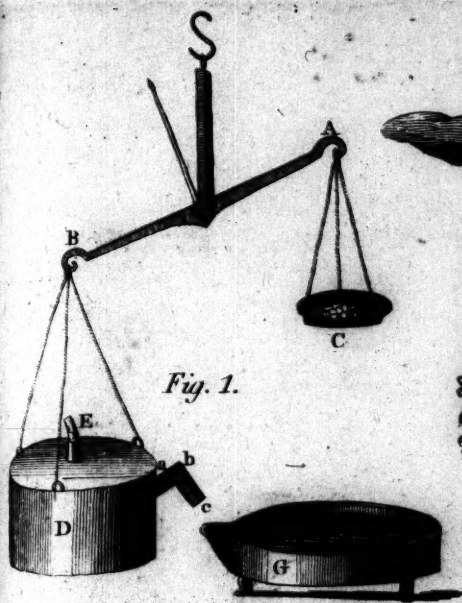


of the University.
I could not, among other motives, to omit the
invaluable advantage I have derived from your
lectures; nor shall I ever forget the friendship
and politeness you have shown me.

I remain with all due respect,

SIR,

Yours, &c.
JOSEPH H. BLACK





ON RESPIRATION.

RESPIRATION is a function so necessary to life, and serves so many important purposes in the animal œconomy, that whatever may contribute to throw the smallest light on so difficult a subject, seems worthy of the attention of the physician. This consideration will, I hope, be a sufficient excuse for making it the subject of a dissertation.

I have had no idea of attempting to clear up all the difficulties which surround this important branch of physiology. I have chiefly endeavoured to investigate the quantity of air usually respired by an adult, and to consider respiration as the chief source of animal heat.

A

These

These subjects have of late occupied the attention of the most celebrated philosophers of the age.

In this investigation I shall take the liberty of paying no attention to any hypothesis, however specious, if it be not founded on the sure test of experiment: Systems and men may pass away, but the laws of truth are eternal.

S E C T. I.

By respiration we mean that function in which, by the alternate dilatation and contraction of the thorax, a quantity of air is received into the lungs, and afterwards expelled from them. The first of these actions is called inspiration, the latter expiration.

Inspiration is chiefly performed by the action of the intercostal muscles, and of the diaphragm; for the ribs and sternum are elevated and pushed forwards by the contraction of the intercostal

intercostal muscles; which, joined to the contraction of the diaphragm, dilates the cavity of the thorax in every direction. Thus the lungs, which closely adhere to the ribs and diaphragm, and passively obey their motions, are extended, and the external air rushes into them through the trachea, by which the equilibrium is preserved. During expiration, all the muscles contracted during inspiration are relaxed; the cartilages of the ribs and the mediastinum, from their inherent elasticity, gradually return to their former situation; hence the ribs are pushed down, the diaphragm elevated, and the cavity of the thorax diminished. This is the cause why the portion of air remaining in the lungs is expelled. Besides, the contraction of the abdominal muscles, and perhaps the elasticity of the lungs, assist expiration.

In this manner the alternate motions of respiration are carried on, from the moment of our entrance into the world, till the vital flame is extinguished. It serves various useful, though
secondary,

(R) secondary, purposes; such as the formation of the voice, the expulsion of the fæces, urine, foetus, &c. This action is in a great measure subject to the influence of the will, though Providence has wisely ordained that it should not be totally dependant on that faculty; when necessary,

(R) Physicians have differed much with respect to the final cause of respiration. Finding it less laborious to build hypotheses than to make experiments, some imagined the blood to be cooled in the lungs; others, that it absorbed nitre in its passage through them. In the second part of this dissertation we shall find the author prove, as clearly as any physiological fact can be proved, the opinion of Black, Crawford, &c. that the lungs are the source of animal heat. Dr. Goodwin remarked also the florid colour the blood receives in its passage through the lungs; and found that when it possessed this florid colour it stimulated the left auricle to contraction, but otherwise not. He does not venture to pronounce what change the blood undergoes, but supposes, "That the chemical change which the blood undergoes in the lungs by respiration, gives it a stimulating quality, by which it is fitted to excite the left auricle and ventricle to contraction;"—and concludes from hence, the chief indication for recovering drowned persons to be inflation. But I think he ought not to recommend dephlogisticated air for this purpose; as, if the principle of accumulated irritability be allowed, its effects would be too violent.

necessary, it is assisted by the action of all the muscles attached to the ribs.

With respect to the different theories on the cause of the first inspiration, and of the alternate motion of the diaphragm and intercostal muscles; as I have nothing to say which would serve to elucidate the subject, I shall pass these opinions over in silence; nor shall I venture, as many ingenious * authors have done, to explain them on mechanical principles. I prefer adopting the opinion of the celebrated Monro, "that it is the work of an all-wise Being." †

Although the study of primary causes is absurd, and although the source of primary motions is beyond our comprehension; nevertheless, an experimental investigation of their mode of action must contribute to the advancement of science.

Thus, though we can never hope to be acquainted with the primary cause of the motion of
the

* Boerhaave, Martin, Whitt, Haller, &c.

† Nervous System, p. 101.

the heart and lungs; any fact, which may throw light on the laws by which they are governed, merits our utmost attention. To this class, in my opinion, belongs the exact estimation of the quantity of air respired, in a healthy state of the system. Though this, at first sight, appears insignificant; yet, if we consider how intimately it is connected with the various theories on respiration, on the passage of the blood through the lungs, on animal heat, and with the cure of (*P*) diseases, which can never be understood without

(*P*) The most violent and obstinate diseases of the system are seated in the lungs; such are pneumonia, asthma, and the different species of phthisis. Dr. Beddoes has lately assigned a theory of the latter disease, very different from that before adopted by physicians. Laying it down as a principle, that oxigene is received into the system during respiration; he supposes phthisis to depend on a certain state of the lungs, by which too much oxigene is received into the system. He supports his opinion by several ingenious analogies; and by the good effects phthical patients have experienced from respiring carbonic gas. We have not, however, as yet, a sufficient number of facts to enable us to determine in what cases the different gasses may be administered with advantage, nor in what proportion they ought to be combined.

Many

without this previous knowledge, it will appear worthy of all the attention we can bestow.

Since

Many celebrated philosophers deny the absorption of oxygen: They think that the combination of oxygen with hydrogen and carbon in the lungs, and the consequent evolution of heat, explain in a satisfactory manner the phenomena of respiration. Dr. Girtanner has made several experiments, with a view of ascertaining this important fact; and their result seems to favour the opinion of the absorption of oxygen: They prove, at least, the presence of oxygen in arterial, and of carbon and hydrogen in venous blood. Dr. Girtanner, not content with this, supposes oxygen to be the principle of irritability: But one consideration, I think, overturns this hypothesis. In the fœtus the blood is of a darker colour than in the adult; yet the more frequent contractions of the heart and arteries, and the greater effect produced by stimuli, prove the greater irritability of the fœtus. The irritability of the system gradually decreases from the first moment of life to that of its extinction; yet, till the moment of birth, the fœtus receives oxygen only from the mother; and that the quantity it receives is small, appears from the dark colour of the placenta. After birth, the lungs perform their functions imperfectly for some time. This is evident from the colour of infants being darker than that of adults; and still more so from the story of the blue boy mentioned by Dr. Beddoes; the right ventricle of whose heart communicated with the left one, and consequently only a small portion of
blood

Since the middle of the last century to the present time, physiologists have endeavoured to ascertain the quantity of air inspired, or how much

blood passed through the lungs: nevertheless, for the first year the boy felt no inconvenience; but at the end of that period the dreadful symptoms commenced, which gradually increased till he died, in his thirteenth year. Does not this case prove, that the change the blood undergoes in the lungs is not so necessary the first year, but that it becomes more and more so as we advance in years? Is it not rational to suppose that oxygen, if it be absorbed, acts as a stimulus on the system? This will explain why Nature has permitted only a small portion of it to be received into the system during infancy; because the irritability being then greater, a smaller proportion of stimulus is required; but, in the same ratio as the irritability is diminished, the stimulus is increased, till we arrive at the acme of vigour, when the quantity of irritability and stimulus may be supposed to be on a par, and the lungs come to their full growth.

But though it is not yet sufficiently proved that oxygen is absorbed, this is not an argument against Dr. Beddoe's method of cure in phthisis. If it is demonstrated that the blood receives a supply of heat in the lungs, the phenomena of phthisis, ascribed to the absorption of oxygen, may be equally well explained by the absorption of heat. This we shall be the more convinced of, if we consider, that oxygenous gas alone undergoes any change from respiration; which perhaps depends on the more perfect combination of caloric with

much the thorax is dilated in each ordinary inspiration *. A late author has come very near the point. As many experiments have been made with this view, and as a great deal has been written on the subject; we shall give, in a few words, an account of the most important experiments, and their result.

The first experiment we shall mention, is that made by Borelli † in the middle of last century. Having breathed through a glass-tube, of

which with oxigene, than with any of the other bases; and the good effects of the combination of hydrogen gas, &c. with atmospheric air, must in this case be explained by a smaller quantity of heat being given out.

Whatever opinion be adopted, it is evident, that the effects of the air respired on the system must vary with the proportion of its constituent principles. As that state of the system called phlogistic, or sthenic diathesis, is always accompanied with an increase of heat, and as it is proved that the lungs are the source of this heat, may we not diminish the heat of the system, by diminishing the quantity of it evolved in the lungs? How far this may be effected in pneumonia, &c. must be determined by future experience.

* Goodwin on Animation.

† Borelli on the Motion of Animals, p. 119 & 133.

which the volume was ascertained, and of which one end was immersed in some bubbles of soap, he found the quantity of air received into the lungs in one inspiration, to be about 15 cubic inches; and towards the end of the experiment, to be between 18 and 20 cubic inches.

It is however clear, that besides the friction and other causes which render this experiment inaccurate, it is liable to objection, as being only the measure of one inspiration. For, as respiration is a function in some degree subservient to the will, it is evident, we can draw no conclusion from the measure of a single inspiration.

The next author we shall mention, who attended to this subject is the celebrated Junin, who came so near the truth, that we shall give his experiment in his own words. "In consequence of an experiment he had made, the celebrated Borelli imagined the quantity of air expelled in an ordinary expiration, to be between

between 18 and 20 cubic ounces. It is however, different, not only in different men, but even in the same man at different times. I made the experiment thus. I suspended a weight to the lower part of a bladder, which I had previously moistened, and having fixed a tube of about an inch diameter in the upper part, I stopped my nostrils, and inspired the air of the bladder gently, during three minutes, the weight remaining all the while on the table. I then plunged the bladder, with the air included in it, and the weight suspended to it, into water contained in a cylindrical vessel. I marked the height to which the water rose. Then having squeezed the air out of the bladder, I again plunged it into the water with the weight. The difference of the height to which the water rose in both these cases, was easily calculated. Having repeated the experiment ten times, and added the quantities together, the tenth part of the sum total, or

" the

(1821)

" the proportional difference of the height to
" which the water rose in both cases was found
" equal to 35 cubic ounces, which is the vol-
" ume of air contained in the bladder, and
" having added about the twelfth part, or 3
" ounces, on account of the condensation of
" the air from the coldness of the water, as it
" was winter, we shall have 38 cubic ounces.
" Besides, a little must be added, both on ac-
" count of the pressure of the water on the
" bladder, and of the moisture which is expell-
" ed with the air, and soon condensed by the
" cooling of the water and of the bladder. I
" then calculated the quantity of air expelled
" by a moderate expiration in the space of
" three minutes, at 40 cubic ounces. In the
" strongest expiration, I expelled 125 cubic
" ounces in the space of a minute. But in a
" very strong expiration, continued till I was
" almost suffocated, I expelled 220 cubic ounces
" of air from my lungs. From whence it follows,

" that

" Memoir, Abridgement of Linnæi, T. 1. p. 412.

" that there is more air in the lungs than can
 " be expelled by an ordinary expiration."

This experiment proves that the quantity of
 air usually expired is equal to 40 cubic inches
 the accurate Halley, Haller, and Sauvages, who
 have repeated these experiments, have agreed
 that the result was the same. But as it is only
 the measure of one inspiration, it lies open to
 the same objection as the former.

The last who endeavoured to investigate this
 matter, was the ingenious Dr. Goddwin; he differs
 very much from those who preceded him in
 this career, and seems to have erred by not suc-
 cessfully varying his experiments. But as his
 reasoning appears at first sight, very accurate,
 it is necessary to give his own words. We
 " must now estimate the quantity of air usually
 " ly respired. This can be pretty accurately
 " done, if we endeavour to breathe from a ves-
 " sel full of air joined by means of a tube, to
 another vessel whence it follows

" vessel full of water; for thus a certain vo-
 " lume of water, equal to the volume of the air
 " inspired, may get into the place of the latter
 " after each respiration; for the volume of wa-
 " ter substituted to that of the air inspired,
 " must be equal to the volume of air consumed
 " in inspiring.

" With this view, I thought on this machine
 " A B C D, G, Fig. 10, of which the vessel D
 " (which I call the pneumatic one) is capable
 " of containing more than 100 cubic inches of
 " air. The tube *a b c* is immersed in water;
 " whilst the tube E is received into the mouth
 " in order to breathe the air inclosed in the
 " vessel. But as the vessel D is suspended to
 " A B, if the tube *a b c* be immersed into the
 " vessel G filled with water, whilst some per-
 " son breathes through the tube E, then a
 " volume of water, precisely the same with
 " that of the air respired, will get into its place.
 " On the other hand, the weight of the vo-
 " lume of water will be shewn by the other

" scale

"scale G; and having calculated, we first find
 "the number of cubic inches of water, which
 "entered into the pneumatic vessel during re-
 "spiration; but it is plain that the number of
 "cubic inches of water, which have passed in-
 "to the vessel, will be equal to the number of
 "cubic inches of air respired." . . . in inspiration
 "An adult, of a middle size, and in good
 "health, endeavoured to respire, as naturally
 "as possible, from the pneumatic vessel. (which was filled with water)
 "At the first inspiration, he inspired 30 cubic inches
 "At the second time, he inspired 31 cubic inches
 "Another person, of the same stature, en-
 "deavoured to breathe out of the vessel.
 "At the first inspiration, he inspired 31 cubic inches
 "At the second, 32 cubic inches
 "But as the difference of the result of both
 "these experiments may be supposed to proceed
 "from the different degree of attention, he thus
 "varied it. "The same man inspired and ex-
 "pired thirty times out of the same vessel, and

as nearly as possible with the same degree of exertion; and, having calculated, we found the average quantity of air of each respiration to be 21 cubic inches.

The same experiment being repeated with the greatest care, the average quantity of each respiration was found to be 3 cubic inches.

Another man, of the same stature, breathed thirty times from the same vessel, and in the same manner; and the average quantity was 31 cubic inches. Hence it follows, that the greatest quantity of air, received into the lungs, during each natural respiration, does not exceed 31 cubic inches; much less than what Hales and Jurin had calculated.

But in the last dissertation, published by the same author, it is mentioned, that a dull kind of pain was felt in the chest, before the man had finished the number of inspirations; and, having removed the tube from his mouth, it

was

was necessary to make a deep inspiration, which seems to prove, that the quantity of air received into the lungs, was not sufficient for the purposes of respiration. This defect is, however, attributed by the author to the imitation of natural respiration, which could not counterbalance the difficulty of raising water contrary to its natural gravity.

As an attempt to breathe in the open air, would not give the measure of an ordinary inspiration from the machine, it was necessary to assist the action of the lungs; by which the quantity received, was much increased, as is evident from the following experiment. "Three persons of ordinary stature, inspired from the pneumatic vessel thirty times successively, and took in as much air at each time, as the sensations in the breast seemed to require. The average quantity of air taken into the lungs at a single inspiration, had marked the number of times the tube from his mouth, having removed the tube from his mouth, it

not differ much from the first, was 12
By the second, 14 cubic inches.
By the third, 12 cubic inches.
Thus Dr. Goodwin concludes the quantity of
air inspired to be equal to 12 cubic inches,
which are dilated by heat to 14; and as the
quantity of air remaining in the lungs, after an
ordinary expiration, is near 109 cubic inches,
he concludes the proportion of the dilatation
of the lungs, before and after a healthy inspira-
tion, to be as 109 to 123.
Although Dr. Goodwin has candidly men-
tioned some of the causes, which render his ex-
periments inaccurate; there are, however, others
which clearly explain the difference between
the result of his experiments and those of Ju-
lin. For besides the resistance of the column
of water to be raised contrary to its natural
gravity as the space of time necessary for the
water to flow from one vessel into another, does
not
count of the many inaccuracies to which it is
liable:
* Goodwin on the Connexion of Life with Respiration.

not differ much from the time of a common inspiration, it is evident that the air in the pneumatic vessel D, fig. 2, must be in some measure expanded, and consequently, the moment the mouth is removed from E, the external air will rush in to restore the equilibrium, and will drive back the column of water in the tube *a b c*, even before a proper quantity can have passed into the vessel D. If the tube *a b c* be large, the quantity of water driven back will be considerable; and if it be small, the space of time necessary for the water to pass from one vessel into another being longer, and the resistance made by the air rushing into the pneumatic vessel, will make the error still greater.

These objections may however be, in a great measure, obviated by the use of valves, and by raising the water to the same height in both vessels. However, in the different experiments I made on this subject, I found that no confidence could be placed in this method, on account of the many inaccuracies to which it is liable:

liable; although it may sometimes be serviceable. I freely confess that it was by chance I first discovered how inaccurate it was,

HAVING measured the volume of the allantois in two different manners, first by the quantity of water it was capable of containing, and afterwards by the diameter and area of it,

As a short time before I had filled some allantois first with inflammable air, and afterwards with my breath, the idea struck me, that by means of a tube sufficiently large, and with good valves joined to it, I could pretty accurately ascertain the quantity of air usually respired; as, on account of the large volume of the allantois, the average quantity of the number of respirations could be easily taken; and on account of the thinness of the membrane, the air could never be much condensed.

I filled the allantois, taking care to keep my nostrils during expiration. The allantois was filled, in repeated trials, by about 20 respirations as natural as possible. So that if you divide

EXPERIMENT

liable; although it may sometimes be serviceable. I freely confess that it was by chance I first discovered how inaccurate it was.

HAVING measured the volume of the allantoid D in two different manners, first by the quantity of water it was capable of containing, and afterwards by the diameter and area of it, I found it to be nearly equal to 2400 cubic inches; and having joined to it the machine A B C, fig. 2d and 3d, consisting of two pretty large tubes joined at right angles, in which are seen the two valves *d* and *e*, so that the air, without any unusual effort, can be inspired through *C*, and expired through *A*; the tubes are large, the valves are made out of an allantoid, and are so thin that the smallest force moves them, I then began to expire, and did not remove my mouth from the tube till I had filled the allantoid; taking care to stop my nostrils during expiration. The allantoid was filled, in repeated trials, by about 56 expirations as natural as possible. So that if you divide

2400 cubic inches, the volume of the allantoïd by 56, you will have 42.8 cubic inches as the average quantity of air usually expired.

Besides the allantoïd E was filled with atmospheric air through C, and the quantity inspired was found nearly the same.

Several persons, of the middle size, repeated this experiment with nearly the same result. The difference was scarcely ever more than one or two cubic inches.

I always took care that the valves were properly fixed on, so that a man may breathe through A B C when joined to the allantoïd.

Surprised at the difference between the results of mine and Dr. Goodwin's experiments, I took two broad vessels, which communicated with each other in the middle, by means of a tube, supplied with valves, constructed in such a peculiar

The volume of air must necessarily be increased or diminished, according to the degree of heat, or of pressure, to which it is exposed.

determined to vary the experiment, to as either

similar manner as to permit the water to pass
 only into the vessel from which I inspired. And
 in order to be able to make many inspirations
 without taking off the mouth, and to prevent
 the external air from rushing in, the machine
 A B C was put into the place of the tube E: on
 account of the breadth of the vessel, the res-
 pendent column did not vary much. By
 pouring water into the open vessel, it was pre-
 served in both at nearly the same altitude. The
 vessel, from which I inspired, was suspended in
 a broad but accurate scale; but the result of
 these experiments made by joining the allantoid
 to the difference between the res-
 pendent column, was so different, sometimes 4, some-
 times 10, sometimes 20 cubic inches, and the
 effort was so painful, that we agreed to prefer
 each other in the middle, by means of a tube
 supplied with valves constructed in such a
 manner that the attention must be necessarily directed
 to what is going on, and that the allantoid can-
 not always contain the same quantity of air. I
 determined to vary the experiment, so as either

tionified the former's self, on to discover if
 there had been any mistake, a method which
 did not appear liable to any objection, and
 which seemed accurate was that proposed by
 Bottrhayd of plunging a man into the water
 up to his chin and judging of the dilatation of
 the lungs from the ascent and descent of the
 water. I know of no person who has tried this
 method. It is necessary that the man
 should be placed in such a manner as not to
 be obliged to move any part of his body above
 the surface of the water; and as it is requisite
 that the ascent and descent of the water should
 be accurately marked; it is evident, that if a
 man be put into the hoghead A B C D fig.
 404 the top of which (there is) a hole large en-
 ough to permit his head to pass out, and if
 the cylindrical vessel E F be well fitted about
 his neck up to his chin, the hoghead being
 filled with water, a quantity of this water, re-
 quired in order to the air required, will alter-
 nately ascend and descend into the vessel E F

This

This quantity may be easily known by applying the altitude to which the water rises by the area of the cylindrical vessel, & by the area of the neck; and if the water between the annulus be taken out and measured, it will give the measure of a common inspiration. And as a man may remain some hours in warm water without any inconvenience, there will be time enough, not only to mark the height to which the water rises, but also to avoid any error which thus made the experiments so obliged to be obliged to the surface of the water, and as it is requisite that the ascent and descent of the water should be accurately marked, it is evident that it is

EXPERIMENT II.

A healthy man, 5 feet 3 inches in height, three feet and some odd inches about the thorax, was closely fast up in the hoghead A B C D, fig. 4, which was filled with water heated to the 60th degree of Fahrenheit's thermometer, as far as that part of the neck which was best suited to measure the difference of ascent and descent. This difference was about

and T

D

the

the height of 1.25 inches. His pulse, both before and after immersion, beat 64 or 65, and his respirations were 14 or 15, in the space of a minute. And they remained the same during the two hours and upwards he remained in the hogthead, without any inconvenience in any respect. During all that time, the difference between the ascent and descent of the

water was 1.75 inches at least. But when he made a deep inspiration, so much air rushed into the lungs, that the water passed out through the cylindrical vessel. But as the area of the cylindrical vessel was 55.41 square inches, and the area of the neck 18; (Z) $55.41 - 18 \times 1.75$

Remains.

25

(Z) It is evident, that if the area of the neck be subtracted from the area of the cylindrical vessel, it will give the area of the space between the neck and the sides of the vessel: now if this area be multiplied by 1.25 inches, the altitude to which the water rises, it will give the area of the space which the water fills between the neck and sides of the vessel; consequently the volume of the water risen, and consequently that of the air respired will be found out.

25=46.76 cubic inches as the quantity of air
usually required by this man. This experi-
ment was thrice repeated with the same result.
But lest there should be any mistake, I made
him breathe through the allantoid.

the hoghead, without any inconvenience in a
ny respect.

THE same man breathed through the ma-
chine A B C, fig. 2, into the allantoid D, which
made a deep inspiration, so much air rushed in-
was found to contain 2700 cubic inches of
air; and in many trials, filled it with 58 ex-
pirations, which gives 46.55 cubic inches as
the quantity of air expired; this calculation is
very near the preceding one. But as it was

manifest

(2) It is evident, that if the air of the neck be sub-
stituted for the air of the lungs, the air of the
lungs will be found to contain 2700 cubic inches of
air, as much as the bladder consequently 2700 cubic inches
of air. The average area was equal to 70 square inches
the length to 30. Thus $30 \times 70 = 2100$. The average
number of this = 2700, but we shall find the same number
2700.

was less than 100; that the respirations of this man were never more than 14 and 14½ in a minute; it appeared probable that he inspired more air than other men of the same stature. In order to ascertain this, and to be able to estimate the average quantity of air inspired, it was necessary to examine the respiration of a man of small stature, who was easily understood from the nature of the experiment. (Experiment 12.)

EXPERIMENT IV.

ANOTHER man, only 5 feet and an inch in height, was shut into the same hogstied, except that the cylindrical vessel was followed by a change. The pulse beat 70 strokes, and the number of respirations was 13 in a minute. The water was heated between the 85th and the 90th of the thermometer of Fahrenheit. The ascent and descent of the water were carefully marked by means of the glass. In the last experiment, the number of respirations in each minute, was equal to 16 of the same man; the quantity of air inspired was however nearly the same, viz. 22.5.

The difference between the ascent and descent, during as long time as he remained there, was 0.95 of an inch, or of his common fractions, the 19th part of an inch. The area of the cylindrical vessel made use of in this experiment, was equal to 57.012 inches, and the area of the neck was equal to 14.0837 inches. Hence (as will be easily understood from the calculation in the former experiment) $57.012 - 14.0837 \times 0.95 = 40.781$ cubic inches, as the quantity of air taken into the lungs, by a common inspiration.

This was confirmed by his breathing into the allentoid, which gave us from 38 to 40 possible inches as the measure of a common inspiration. So that if you take half the quantities of the 2d and 4th experiments, you will have 43.77 cubic inches as the average quantity of air expired. The ascent and descent of the water were carefully marked by means of the glass-tube G, fig. 4, on which a scale of degrees was cut; this was not however quite so useful as

may

may be expected, on account of the short time
the water remained stationary. But when
something had been subtracted*, on account
of the attraction between the water and the
sides of the tube, the measure of the ascent and
descent was pretty accurate. So that if it may
seem necessary to subtract any thing + more on
account of the dilatation of the air by the heat
of the lungs, we may still compute the quanti-
ty of air inspired, at 40 cubic inches.

Dr Goodwin supposes, that only 100 cubic
inches of air remain in the lungs after an ordi-
nary expiration, and that the proportion of
their dilatation after an ordinary expiration is

100 will give 120 as the quantity of air re-

For although the ascent and descent was in
ready 1.3 inches in the experiment, and in the expe-
riment equal to 1 inch; yet, on account of the attraction be-
tween the water and the sides of the vessel, the first was only
estimated at 1.25 and the latter at 0.95.

But as atmospheric air is dilated by every degree of heat
about 1.17: 40 cubic inches, heated 40° will be increased
in volume by 3.38 cubic inches. For $\frac{40}{100} \times 40 = 16$
Thus $43.77 - 3.38 = 40.39$.

may be expected on account of the short time
to that of their dilatation after an ordinary
the water remained stationary. But when
spirations 100 to 123 *. But I remarked
that many men, especially when shut up in
hogheads, after an ordinary expiration, could
still expel 70 cubic inches of air from their
lungs. Hence after such an expiration, only
39 cubic inches of air will remain in the lungs.
Without doubt then, as appears from another
experiment †, Dr Goodwin supposes that 100
cubic inches of air remain in the lungs after an
extraordinary expiration. But as we have
found 70 cubic inches to be the difference be-
tween an ordinary and an extraordinary expi-
ration, this number added to the former or to
109, will give 179 as the quantity of air re-

maining in the lungs after an ordinary expira-

tion. Now the former experiments shew that

the quantity of air expelled by an ordinary ex-
piration is equal to 40 cubic inches, conse-

quently
about 117.7 : 40 cubic inches, heated 40° will be increased
in volume by 3.38 cubic inches. For $117.7 \times .028 = 3.38$
Connexion of Life with Respiration, p. 37.
Thus $43.77 - 3.38 = 40.39$

† Id. p. 46.

quently if this be added to 179, we shall have 219 cubic inches as the quantity of air contained in the lungs before expiration. Hence the dilatation of the lungs before and after an ordinary expiration, will be as 219 to 179; or in other words, the thorax will be increased by a quantity of air nearly equal to a cube of $3\frac{1}{2}$ inches which, if we consider how much the thorax and abdomen are dilated by an ordinary inspiration, seems very trifling. The difference between an extraordinary expiration, or one made with some effort, and an extraordinary inspiration, I often found to exceed 100 cubic (T) inches.

SECT.

(T) Dr Goodwin expelled the air from the lungs of three dead bodies of an ordinary size, and in one found the quantity of air 272 cubic inches, in another 250, and in the third 262, but these subjects were hanged; and, as he remarks, persons under the influence of fear make a deep inspiration, and cannot afterwards expel the air on account of the cord tied round the trachea.

S. E. C. T. III.

And even then the lungs, before and after expiration, are much more dilated than Dr Goodwin had supposed; this by no means invalidates his objection against Haller's theory on the facility or difficulty of the passage of the blood through the lungs in the different stages of respiration. And although I should by no means pretend to decide on the subject; yet it is evident from Dr Goodwin's experiments, particularly when he induced an artificial hydrothorax (S), that the blood can pass through the

E lungs,

TO 12

(S) The chief use of respiration, according to Haller, is to dilate the lungs so as to permit the free passage of the blood from the right ventricle to the left auricle; but as Dr Goodwin computed, that only 14 cubic inches of air were taken into the lungs by each ordinary inspiration, and as he found, that though he diminished the cavity of the lungs, by creating an artificial hydrothorax, the animal felt no great inconvenience; he hence justly concludes, that though respiration should be suspended, the free passage of the blood from the

right

lungs, whilst they are in a greater state of collapse than ever happens during natural respiration. I cannot then hesitate in coinciding with him in opinion, " That the dilatation of the lungs is not the final cause of respiration."

ANIMAL HEAT.

I SHALL next venture to examine how the ascertainment of the quantity of air usually respired may throw light on the generation of animal heat, and on the quantity of heat generated in a given time.

Mayow was, I believe, the first who hinted that

right to the left side of the heart would continue, if there had not been some other cause. Dr Menzies's experiments, though they prove the quantity of air inspired to be much greater than Dr Goodwin had supposed, do not destroy his arguments against Haller's theory on the use of respiration; for Dr Menzies proves that between $\frac{1}{4}$ and $\frac{1}{2}$ of the air contained in the lungs is taken in by a single inspiration, and Dr Goodwin proves that the lungs may be reduced to $\frac{2}{3}$ of their volume, with preventing the passage of the blood.

that animal heat was generated in the lungs; this was during the last century. Since his time, many other hypotheses were framed and died away; such as, that it depended on the friction of the fluids against the sides of the vessels, on fermentation, &c.; and that the air rather served to cool the blood.

But the celebrated Dr. Black made it appear probable, more than 20 years ago, that animal heat was generated in the lungs, and supported his opinion by many strong arguments; viz, that air is changed in the same manner by respiration as it is by combustion, which he beautifully demonstrated by passing air changed by each mode, through lime-water; next that the degree of heat in different animals is in proportion to the quantity of air they mephitize; and that the foetus in utero does not generate heat till it has begun to breathe.

The same philosopher also, by remarking the different capacities of mercury and water for heat, and by estimating as much as it can be

done,

done, the heat of various bodies, laid the foundation of the doctrine of comparative heat. Drs. Jurin and Crawford prosecuted the subject farther. The latter, assisted by Dr. Black's discoveries, clearly proved, in my opinion, that animal heat is derived from the change induced on the air in the lungs (*T*), by which its capacity for heat is diminished, and a quantity of sensible heat is given out. Dr. Crawford has laid it down as a principle, that the quantity of heat, which is yielded by pure air, when it is converted into fixed air, and aqueous vapour, is such

(17) Dr Crawford concludes from his experiments, that animal heat depends on elective attraction; oxygenous gas is received into the lungs; the blood is returned from all parts of the system impregnated with carbonated hydrogen; carbonated hydrogen has a greater affinity for oxygenous gas than for the blood; consequently a double decomposition takes place, the hydrogen unites with the oxygenous gas, and the heat separated from the oxygenous gas unites with the blood. The arterial blood having thus received a supply of caloric, gives it out in the course of circulation, and receives hydrogen in the capillary vessels.

such (as if it were not dissipated) could raise the air or vapour so changed to 4 times *.

On account of the nicety of his experiments with respect to the different capacities for heat of the different kinds of air, and on account of the uncertainty of the lowest degree of heat, which he by no means imagines to have ascertained †, many objections have been made to his doctrine, and some persons have totally rejected it.

But if it can be proved that animal heat is not only generated in the lungs, but that the quantity of it thus generated can be estimated independently of any theory of the nature of

heat

* This paragraph is rather obscure in the original, but the words of Dr Crawford himself are, "The quantity of heat yielded by pure air, when it is converted into fixed air, and aqueous vapour, is such (if it were not dissipated) as would raise the air and vapour, so changed, to more than four times the excess of the heat of red hot iron above the common temperature of the atmosphere."

† Crawford on animal heat, p. 375.

heat itself, and without any calculation of the different capacities of the different airs or of the lowest degree of heat; it will serve to confirm the conclusions of the ingenious Dr. Crawford, as the result will be found the same, though arrived at by a different mode of reasoning.

1st, The following circumstances render it probable, that animal heat is generated in the lungs; those animals alone, which have lungs, and breathe air, can preserve themselves in a degree of temperature superior to that of the surrounding bodies (*W*); and their degree of heat

(*W*) Another question intimately connected with this, is the power of animals to generate cold, or to speak more philosophically, to preserve themselves below the temperature of the medium they live in, if it be excessive. The increased evaporation had been supposed the cause, but as it happens equally in a dry or a moist atmosphere, Dr Crawford concludes that some other cause must contribute to it; his experiments prove that less air is mephitized by respiration in a warm than in a cold medium, in other words, that less hydrogen is given out by the lungs in the former than in the latter case; but this hydrogen he supposes to be combined with the blood in the capillary

heat is in proportion to the volume of their lungs, and to the quantity of air inspired in a given time. Thus birds, whose lungs are proportionably larger than those of other animals, and who mephitize more air in a given time, are found to have very warm blood. On the contrary, fishes and amphibious animals have their blood more or less warm according to the quantity of air they require.

2dly, It will appear that animal heat depends on the change the blood undergoes in the lungs from the action of the air, if we attend to what takes place in combustion. For in the combustion of wax, of coal, which cannot be kept up without vital air, we find the
air

capillary vessels, and heat consequently given out; hence less hydrogen is combined with the blood in a warm temperature, and consequently less heat given out; in other words, a degree of cold is produced. By evaporation the surface of the body is cooled, and by the union of a smaller proportion of oxygenous gas with hydrogen in the lungs, a smaller quantity of heat is given out there.

air in which the combustion had taken place to be destructive to animal life, incapable of supporting the flame of a candle, and a part of it to be converted into fixed air. The very same, as Dr. Black has proved, happens in respiration. Besides from Lavoisier's accurate analysis of atmospheric air, 73 parts of it consist of nitrogene gas, and 27 of oxygenous; and the latter of these alone is changed by combustion or respiration (X). But, in the combustion of coal, it is proved that this part of atmospheric air is entirely converted into fixed air, and that a pretty large quantity of heat is generated. As in respiration the air undergoes a similar change,

(X) This is proved by the following experiment. If an animal be placed in a jar inverted over water or mercury, in some time it will perish; the air will be diminished in volume, and the portion remaining will be found to consist of phlogistated or nitrogenous gas, with a small portion of oxygenous gas diffused in such a manner as to be unfit for the purposes of respiration.

change, we are authorised to conclude, that an equal quantity of heat is evolved.

But this is not supported by analogy alone. For the experiments of Crawford *, Lavoisier and De la Place †, prove that when equal quantities of air are vitiated by respiration and combustion, equal degrees of heat are evolved. The celebrated Crawford found, that if an animal was shut up in a vessel surrounded with water, and protected from the access of air by very soft wool, 100 measures of air, each containing an ounce, vitiated either by this animal's respiration, or by the combustion of wax or coal, communicated the following quantities of heat to 31 lb. 7 oz. of water.

F

100

* On animal heat.

† Memoire sur la chaleur.

‡ The weight meant here, and in other parts of the dissertation, is that usually called troy weight.

	Degrees,
100 measures } combustion of coal, gave	19.3
of vital air vi- } <u>wax,</u>	21
riated by the } respiration of a guinea-pig,	17.3

But as each degree of Crawford's thermometer makes only one tenth of a degree of Fahrenheit's, it is very evident, if a thousand such measures were vitiated by the combustion of wax, and the respiration of a guinea-pig, that the difference between the quantities of heat given out to 31 lb. 7 oz. of water will make 3.7 degrees of Fahrenheit's; the difference between the quantities given out by the combustion of coal, and the respiration of a guinea-pig, will be equal to 2° of Fahrenheit's thermometer, or the heat communicated to the water by the guinea-pig, will be two degrees less than that communicated by the combustion of coal.

But from the series of experiments of Lavoisier

● On animal heat, p. 351.

fier and De la Place *, it appears, that the heat communicated by the respiration of a guinea-pig, when equal quantities of air were vitiated, was greater than that generated by the combustion of coal, in the proportion of 13 to 10.3. I am sorry I cannot repeat these experiments with the Calorimeter †, the only method of doing it accurately. But as cotemporary authors have supposed the degree of heat said to be communicated by the guinea-pig, to be somewhat exaggerated, and as the result of their experiments was almost the same, we think we may safely take the average number as a rule, since the difference probably proceeded from the different construction of the instruments employed, and we conceive it to be sufficiently demonstrated, that when equal quantities

* *Memoire sur la chaleur*, lu 1783.

† An instrument which measures the degree of heat by the melting of ice, and is described in *Lavoisier's Elements of Chemistry*.

titles of air are vitiated, whether by respiration or the combustion of coal, nearly equal quantities of heat are generated.

As the quantity of heat generated when a given quantity of fixed air is produced by the combustion of coal, has been lately demonstrated by Lavoisier *; and as the quantity of air usually respired has been ascertained, it is evident that the quantity of heat (*F*) generated in the

* Elements of Chemistry, p. 101.

(*F*) The experiments of Monf. Lavoisier prove that $\frac{4}{5}$ of the oxygen which disappears in respiration are consumed in the formation of carbonic acid gas by the combination of oxygen with the carbone of the blood, the remaining $\frac{1}{5}$ is either absorbed by the blood, or is expended in the formation of water with the hydrogen of the blood; the latter opinion seems probable to Monf. Seguin, (Vid. *Medicine éclairée par les sciences physiques*, tom. 1.) who thinks that the arterial blood becomes venous in the extremities of the arteries by absorbing hydrogen, and that vice versa the venous blood becomes arterial by giving out its hydrogen in the lungs; this hydrogen he imagines to hold a considerable quantity of carbone in solution. Monf. Seguin's opinion of the cause of animal heat scarce differs from Dr Crawford's; oxygen gas he thinks is decomposed in the lungs,

on

the lungs, in any given time, could be also found out, were we able to estimate the quantity of fixed air, in air which has been once breathed. The following experiments were made with this view.

EXPERIMENT I.

A quantity of air only once respired, was pressed out of the allantoid E, by means of the curved tube *a b c*, in which there is a cock at *d*, into the bottle F G, till it was filled. Left any part of the fixed air should have been absorbed by the water, some oil was poured into the bottle before the air was introduced. The bottle was then removed from the vessel A C, and

on account of the affinity of carbonated hydrogen for oxygen being greater than that of oxygen for caloric, and of carbonated hydrogen for the blood; water and carbonic acid gas are formed; caloric consequently given out, which unites with venous blood, the capacity of which for heat is increased by losing its carbonated hydrogen, and thus venous blood becomes arterial.

and was inverted into a vessel, fig. 6, filled with caustic alkali; the barometer was in the mean time attended to, and the degree of heat marked by the thermometer, fig. 7. The air in the bottle was shaken, and left in contact with the caustic alkali till all the fixed air was absorbed. Then the bottle was put into the vessel A C, and plunged into the water, till the caustic alkali in the bottle, and the water in the vessel were at the same height. Then having stopped the bottle with a cork, and placing it on its bottom, it contained a quantity of caustic alkali, which accurately weighed gave the quantity of fixed air absorbed, after some corrections being made on account of the ascent and descent of the mercury in the barometer, and the difference of temperature in the bottle.

Thus the bottle contained 2038.5 cubic inches; the temperature of the air of the bottle was 59° of Fahrenheit's thermometer, the altitude of the mercury in the barometer was 29.87 inches; the temperature of the air after two days,

days, was 57.5° ; the altitude of the mercury in the barometer was 29.37 inches; the caustic alkali of the bottle weighed 5 lb. 9 $\frac{1}{2}$ oz. equal to 131.2713 cubic inches. But on account of the ascent of the barometer, it is necessary to subtract 20.9 cubic inches; and 6.471 cubic inches, because the air in the bottle was rendered 1.5° of Fahrenheit's thermometer, colder.

For as the volume of elastic fluids is in the inverse proportion of the weight pressing on them, $29.37 : 29.07 = 2038.5 : x = 2017.6$; and $2038.5 - 2017.6 = 20.9$.

And as atmospheric air is expanded by 11.7 of its volume for every degree of Fahrenheit's thermometer, $11.7 \times 1.5^{\circ} = 6.471$. So $131.2713 - 20.9 - 6.471 = 103.9$, the number of cubic inches of fixed air in 2038.5 inches of air once respired; a quantity less than $\frac{1}{4}$ th of the whole; for $103.9 : 411.6 = 15.6$.

But lest any part of the fixed air should be absorbed, oil was used instead of water.

EXPERIMENT

EXPERIMENT II.

The bottle A, fig. 6, was filled with air once respired and passed through oil: A contains 179.812 cubic inches.

The thermometer and barometer were as in the last experiment. The caustic alkali after two days, weighed $6\frac{1}{10}$ ounces, and measured 11.451 cubic inches. But $29.37:29.07 = 179.812 : x = 177.973$. And $179.812 - 177.973 = 1.839$; and $1\frac{1}{4} : \frac{1}{2} \times 1.5^\circ = 0.5707$.

Thus $11.451 - 1.839 - 0.5707 = 9.041$, the quantity of fixed air found in 179.8 cubic inches of air once respired; for $\frac{1}{7} \cdot \frac{8\frac{1}{2}}{10} = 19.8$.

The same experiment being repeated with the same bottle, 3 oz. and 1 drachm only of caustic alkali, measuring 5.9148 cubic inches, were found in the bottle. But as during this time the mercury in the bottle has fallen the $\frac{1}{10}$ of an inch, 3.817 cubic inches must be added to that quantity. And as the air in the

bottle

bottle was found $\frac{1}{4}$ of a degree colder towards the end than at the beginning of the experiment, 0.3044 parts of an inch are to be subtracted from it.

For $29.2 : 29.82 = 179.812 : x = 183.629$; and $183.629 - 179.812 = 3.817$; and $\frac{179}{179} \cdot \frac{1}{4} \times 0.8 = 0.3044$.

So that $5.9148 + 3.817 - 0.3044 = 9.427$.

And $\frac{179}{9} : \frac{812}{427} = 19.07$.

In frequently repeating those experiments through the large and small bottles, whilst the state of the thermometer and barometer were carefully attended to, the greatest variation was 20.1. So that the average number will be 19.6.

Some experiments were also made with a view of discovering the quantity of fixed air in the room where these experiments were made; but the quantity of fixed air in 2038.5 cubic inches of air respired, was found so small as to be scarce perceptible. If any calculation be ven-

G

tured

tured on, the quantity of fixed air may be estimated at the $\frac{1}{16}$ or $\frac{2}{100}$ part of air once respired.

S E C T. IV.

THUS, if the quantity of air commonly inspired, be estimated at 40 cubic inches, and the number of respirations at 18 in a minute, 720 cubic inches will be inspired in the space of a minute; of which quantity only the $\frac{21}{100}$ or 194.4 cubic inches consist of vital air, the only constituent of atmospheric air changed by respiration. But the $\frac{1}{16}$ parts only of atmospheric air are changed in each respiration. Hence 36 cubic inches of fixed air are generated in the space of a minute in the lungs of a middle-sized man, or 51840 cubic inches in the space of a day; a quantity of air weighing 22865.5 grains, or 3.9697 lb. troy weight. And as the celebrated Lavoisier has accurately calculated, that for every pound of fixed air generated by the

the

the combustion of coal, a quantity of heat was evolved which would melt 27.02024 lb. of ice; and as the same quantity of heat is generated in air, vitiated either by respiration or by the combustion of coal, it follows, that the quantity of air vitiated daily in the lungs of an ordinary man, will give out nearly as much heat as would melt 107.2 lb. of ice. For $27.02024 \times 3.9697 = 107.2622$. But as a portion of this heat is carried off in the air expired under the form of sensible heat, and as a portion is employed in the formation of vapour, or is rendered latent, these quantities can be calculated in the following manner.

As a cubic * inch of atmospheric air weighs 0.32112 parts of a grain, 40 cubic inches will weigh 12.8448 grains. But the $\frac{1}{150}$ parts of this quantity being converted into fixed air, if the whole remains free from moisture, it will gain 0.19794, so that it will weigh 13.04274 grains.

* Lavoisier's Elements of Chemistry.

grains. Hence the air expired during the space of a minute will weigh 234.7693 grains, or that expired during an entire day will weigh 338067.82 grains, or 58.692 lb. But if air of this kind be supposed to have the same capacity for * receiving heat as water, it follows that the same degree of heat which would raise 58.69 lb. of water some degrees, would also raise the air to the same degree. But the quantity of heat requisite to raise 58.69 lb. of water to the 66th degree of Fahrenheit's thermometer, is equal to that which would melt 27.6692 lb. of ice; for since 140 degrees of heat become latent in the formation of each pound of water, $\frac{58.69 \times 66^\circ}{140} = 27.6692$.

Consequently the same degree of heat will be daily evolved in the lungs under the form of sensible heat.

The important discovery of latent heat shews that a great quantity of heat is absorbed during the

† Crawford supposes it to have less capacity for heat.

the formation of vapour, without an increase of temperature. And it appears from the experiments of Mr. Watt, that the heat thus absorbed or rendered latent, would raise the temperature of a body of the same specific gravity and capacity for heat as water, although it could not be converted into vapour, 960 degrees more than before.

I weighed accurately a large allantoid, and then fixed it on the above-mentioned machine; the allantoid being empty, I filled it, by means of the machine, with air expired. After the allantoid being left some time to cool, and then carefully weighed, it was found to gain two grains by every expiration; which coincides remarkably with an experiment made by Dr. Hales, who when he had breathed through some moistened diaphragms, found they gained six grains in the space of three minutes*. Although in the last experiment, in which he
breathed

* Vegetable Statics, vol. 1. p. 268.

breathed through burning coals, which absorbed the fixed air of the lungs and the moisture, he concludes the humidity to have been greater. Thus if you subtract two grains of aqueous vapour every minute, without attending to what may be subtracted on account of the difference between the weight of fixed and vital air, you will subtract 6 oz. or 0.5 of a lb. daily. But if the temperature of a body incapable of being evaporated, of the same capacity for heat as water, weighing 0.5, be increased 960° , this heat will dissolve 3.42854 lb. of ice. For $\frac{960 \times 0.5}{140} = 3.4285$. But as Dr. Crawford computes the relative heat of aqueous vapour at 1.55, a quantity of heat will be subtracted which would dissolve 1 lb. and 0.8856 hundredth parts of ice more than if its capacity for heat had been the same with that of water; or in other words, all the heat daily consumed in the lungs in the formation of aqueous vapour, would dissolve 5.3141 lb. of ice. So that all the heat, daily evolved in the lungs,

under

under the form of sensible heat and vapour, would dissolve 32.9833 lb. of ice. For $27.6692 + 5.3141 = 32.9833$.

But as it was before demonstrated, that in the space of a day a quantity of heat was evolved, by the change induced in the air, in the lungs, which would dissolve

	107.2622	} lb. of ice.
Subtract from thence	32.9833	
	<hr/>	

There will remain 74.2789 lb. of ice, which would be the quantity capable of being dissolved by the heat daily evolved in the lungs of an ordinary man. But it is necessary the blood should absorb the heat, because it is exposed to its action in the lungs for the space of some hundreds of square feet *, and because this fluid is admirably calculated to diffuse heat through all the body.

The degree to which the blood is heated in
its

* Hale's Statical Essays, vol. 1. p. 239, and the celebrated Dr Monro's lectures.

its passage through the lungs, may be estimated in the following manner. For as the quantity of blood, which passes through the lungs in the space of a minute, may be estimated at 8 lb. if we suppose the heart to propel an ounce and a half in each systole; and if we suppose blood to have an equal capacity for heat with water, it is evident that the quantity of heat which would make 8 lb. of water rise to any given point, would also make 8 lb. of blood rise to the same point. But as the quantity of heat absorbed during the space of a day by the blood, has been demonstrated sufficient to dissolve 74.27 lb. of ice; the quantity absorbed each minute could only dissolve the 0.05158 of a lb. of ice. Besides, since 140° of heat are requisite to liquefy 1 lb. of ice, or become latent, $1 : 140 = 0.05158 : x = 7.22^{\circ}$, or the quantity of heat requisite for dissolving 0.05158 of a lb. of ice, would increase the temperature of a lb. of water by 7.22° , or 8 lb. by 0.90265 of a degree of the thermometer of Fahrenheit. Thus
the

the heat generated each minute in the lungs of an ordinary man, would raise the temperature of the blood passing through them in that space of time, by 0.90265 of a degree, if blood had an equal capacity for heat with water.

But as Dr. Crawford concludes that the (V) comparative heat of venous blood bears the same proportion to that of water as 0.8928 to 1, it is evident, that the same degree of heat which would raise venous blood one degree, would raise the same quantity of water only 0.8928 of a degree, or that their temperatures are in the inverse proportion of their capacities for heat.

Thus the quantity of heat which would raise 8 lb. of water 0.90265 of a degree, would raise

H the

(V) Dr Crawford's experiments prove that the blood which passes from the lungs to the heart by the pulmonary vein, contains more absolute heat than that which passes from the heart to the lungs by the pulmonary artery. This amounts to a demonstration that the lungs are the source of animal heat.

the same quantity of venous blood 1.01103° ; for
 $0.8928:1.0000=0.90265:\frac{.90265 \times 1}{0.8928}=1.01103.$

And as he lays down, that the capacity of venous blood for heat is increased by its becoming arterial, in the proportion of 0.8928 to 1.03; if venous blood is raised 1.01103° by the heat evolved in the lungs, it follows that arterial blood will be raised only 0.8763: for as $1.03:0.8928=1.01103:x=0.8763.$

Thus 0.13468 parts of a degree of Fahrenheit's thermometer, equal the quantity of heat which becomes latent, although the capacity of the blood for heat is increased in the same proportion; for $1.01103-0.87635=0.13468.$

Therefore the blood, in its passage through the lungs, gains 1.01103, or more than one degree of Fahrenheit's thermometer, and its temperature is increased 0.8763 of a degree. From whence it follows that the blood in the left side of the heart is warmer than that in the right by $\frac{1}{16}$ of a degree. This seems confirmed by an experiment

experiment of Hunter's on a live dog, which I shall give in his own words.

" The ball of a thermometer being introduced two inches within the rectum, the quicksilver rose to 100° and a half exactly. The chest of the dog was then opened, and a wound made into the right ventricle of the heart, and immediately, on the ball being introduced, the quicksilver rose to 101° exactly. A wound was next made some way into the substance of the liver; and, the ball being introduced, the quicksilver rose to 100° and three quarters. It was next introduced into the cavity of the stomach, where it stood at 101° . All these experiments were made in the space of a few minutes.*"

SECT.

† Animal economy, p. 102.

S E C T. V.

Thus it has been shewn, not only that animal heat is generated in the lungs, but that the quantity so generated can be determined by a method, which has no connexion with any theory on animal heat, nor with the different capacities of fixed and vital air for heat.

This method is founded on the two following propositions, which we presume, have been demonstrated by our experiments.

1st, That nearly equal quantities of heat are evolved, when equal quantities of vital air are vitiated whether by the combustion of coal or by the respiration of animals.

2dly, That the quantity of fixed air generated in the lungs in any given time, can be easily determined by knowing the quantity of fixed air in air once respired.

But

But if the quantity of air commonly respired had been so small as Dr Goodwin had supposed, it is evident, that so small a proportion of it would have been changed in the lungs, that this organ could not be considered the source of animal heat. And in fact, several objections were made to Dr Crawford's theory on account of the experiments of Dr Goodwin, and some others made by the celebrated De la Metherie, who estimates the quantity of air commonly inspired at 8 or 10 cubic inches, and supposes therefore, that not more than $\frac{1}{4}$ an inch of fixed air is generated.

But as Mons. De la Metherie measured only one respiration, and that without much accuracy, there is no necessity of dwelling any longer on this topic. But from the above experiments and calculations we necessarily conclude, that the quantity of heat generated in the lungs is sufficient to compensate for its continual loss. We cannot therefore sufficiently admire the infinite wisdom of the supreme Being,

who

who has made heat be generated in the lungs from that very element, which draws off heat from every other part of the body. We cannot but admire also the diffusion of heat through the entire system by means of the blood. Hence we see the reason of filling the lungs of drowned persons with air; whether filling the lungs with air be the most efficacious method of restoring the proper degree of heat to the vital parts, or whether it be a stimulus to the heart; the motion of which perhaps ceases in a great measure from the loss of heat. This is rendered probable, not only by the blood being hotter in the lungs, and therefore in the left side of the heart, than in other parts of the body; but also by an experiment of the ingenious Dr Gardiner, which I shall give in his own words. " Some years ago, I cut out
 " the heart and part of the large vessels of a
 " turtle, with a view to examine the structure
 " of the parts, and the circulation of the blood
 " in that animal. Having wiped off the blood
 " and

" and other moisture, the heart was wrapped
 " up in a handkerchief; but engagements in
 " the way of my profession obliged me to post-
 " pone my curiosity till about 6 or 7 hours af-
 " ter it was cut out. When I examined it,
 " there appeared not the least signs of life. It
 " was much shrivelled and dried. But, upon
 " putting it in water nearly milk warm, it
 " plumped up, and some parts of it acquired a
 " tremulous motion. Laying it on the table,
 " and pricking it with a large needle, it palpi-
 " tated several times. The palpitation renew-
 " ed as often as the needle was pushed into its
 " substance, until it became cold, when it
 " seemed to be insensible to every stimulus.
 " But after warming it again in water, it reco-
 " vered its irritability, and repeated its palpi-
 " tations on the application of the needle.
 " Though no motion could be excited in it by
 " any stimulus when cold, yet it moved several
 " times after being macerated in warm water.
 " This evidently shews the necessity of heat
 " and

“ and moisture for maintaining the full powers
“ of the living principle *.

As the air may be made to act on blood drawn out of the body and quite cold, it is probable that heat may be generated in the lungs of men who had been a long time immersed in water.

As in some instances, men who had been only a short time (N) under water, or exposed to

* Animal economy, p. 46.

(N) Mr Coleman differs widely from Dr Goodwin about the cause of death from suspended respiration: According to his opinion, the effects are nearly the same whether the animal perish from hanging, drowning, or breathing a noxious gas; in all these cases the lungs were found collapsed, and the proportion of blood in the right side of the heart was to that of the left, as 13 to 7. It is worthy of remark, that the heart was much less irritable in those who perished in any of the noxious gasses, than in those who perished from hanging or drowning.

The proximate cause of death from hanging, drowning, or respiring noxious gasses, he supposes to be an obstruction in the minute pulmonary vessels, arising from a collapse of the lungs: This, he thinks, happens in every instance of death from any of these three causes.

Although Mr Coleman's experiments are numerous and highly ingenious, yet Dr Goodwin's opinions on the subject are more generally adopted, particularly in this University.

to mephitic air, did not recover, though every attempt was made of inflating their lungs in the usual manner; and as it appeared probable that their death was owing to a small quantity of water or mephitic air, which had insinuated itself into the pulmonary vesicles, so as not to be got out by the common method; would it not be proper, in such a dreadful instance, to make one side of the lungs collapse by a puncture into the thorax? For thus the water or mephitic air would be expelled by a few inflations, and pure air would be closely applied to the pulmonary vesicles. It is also evident, that little danger would occur from the air being admitted between the lungs and pleura, if we took care to force it out, as appears from many cases of emphysema. We must not however indulge in conjectures, when it is possible to ascertain the fact by experiment: as in this entire dissertation we have cautiously avoided theory, except it was of manifest advantage. Many corollaries may be deduced from the above-

mentioned calculations, such as the increased heat of the body from exercise, and whatever propels a greater quantity of blood through the heart in a given time. But as we have already transgressed the bounds prescribed for this kind of dissertations, we shall say no more on the subject.



F I N I S

ERRATA.

Page 30. line 3. of the 2d Note, loco $\frac{40}{271} \times 40^\circ$, read $\frac{40}{271.5} \times 40^\circ$.

48. 9. loco $\frac{1}{7} \times 12$ read $\frac{11}{7}$.

12. loco $\frac{17}{9} \cdot \frac{8}{11} = 19.8$, read $\frac{17}{9} \cdot \frac{8}{11} = 19.8$.

49. 5. loco $\frac{17}{11} \cdot \frac{8}{7} \times 0.8$, &c. read $\frac{17}{11} \cdot \frac{8}{7} \times 0.8$.

50. 2. read may be estimated at $\frac{1}{15}$ or $\frac{1}{16}$ part of air once respired.

51. 13. loco 276692 lb. of ice, read 27,6692 lb. of ice.

56. line the last, read by $\frac{1}{16}$ of a degree.

Speedily will be Published,

PHYSIOLOGICAL
DISSERTATION

ON THE

FUNCTIONS OF THE PLACENTA.

Translated from the Latin of Dr. Janner.

"From these Facts and Observations we may infer, that the
"Placenta is an organ for giving due oxygenation to the
"Blood of the Fetus, which is more necessary, or at least
"more frequently necessary, than even the supply of Food."

ZOOONOMIA, Pag. 477.



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